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On the Origins of the Task Mixing Cost in the Cuing Task-Switching Paradigm

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Poorer performance in conditions involving task repetition within blocks of mixed tasks relative to task repetition within blocks of single task is called mixing cost (MC). In 2 experiments exploring 2 hypotheses regarding the origins of MC, participants either switched between cued shape and color tasks, or they performed them as single tasks. Experiment 1 supported the hypothesis that mixed-tasks trials require the resolution of task ambiguity by showing that MC existed only with ambiguous stimuli that afforded both tasks and not with unambiguous stimuli affording only 1 task. Experiment 2 failed to support the hypothesis that holding multiple task sets in working memory (WM) generates MC by showing that systematic manipulation of the number of stimulus–response rules in WM did not affect MC. The results emphasize the role of competition management between task sets during task control.

Keywords: task switching, switching cost, mixing cost, task ambiguity, working memory

One of the prominent questions in psychological research concerns behavior control. Whereas the behaviorists' tradition has ascribed most behavioral control to the environment, recent cognitive theorists have emphasized more internally driven, top–down forms of control. This idea of cognitive control involves concepts such as goal-directed behavior, initiation, executive control processes, and so forth. A key concept in the context of top–down control is the *task-set*. According to Rogers and Monsell (1995), the control of task-sets is manifested in the ability to configure processing resources to perform one rather than another of the many cognitive tasks that a stimulus affords.

A common paradigm for studying task-set control is the task-switching paradigm. In the original version of this paradigm, performance in blocks of trials in which a task is repeated is compared with performance in blocks in which the participants switch between two different tasks (Allport, Styles, & Hsieh, 1994; Jersild, 1927; Spector & Biederman, 1976). More recent studies have used a modified paradigm that makes it possible to contrast task-switch trials and task-repetition trials within blocks of mixed tasks (e.g., De Jong, 1995b, 2000; Goschke, 2000; Mayr & Keele, 2000; Meiran, 1996; Rogers & Monsell, 1995). In most cases, switching tasks is accompanied by a robust performance cost, seen both in reaction time (RT) and error rates, indicating switch cost.

A recent conceptualization that incorporates knowledge from the variety of task-switching paradigms elaborates and sharpens the understanding of the switch mechanisms by differentiating between several cost components. The difference in performance between switch and repetition trials (in mixed-tasks blocks) is termed *switching cost*, and the difference between repetition trials (in mixed-tasks blocks) and single-task trials (in pure blocks) is termed *mixing cost* (Fagot, 1994; Kray & Lindenberger, 2000; Los, 1996; Meiran, Chorev, & Sapir, 2000). These two cost components compose the *alternation cost*—the difference in performance between switch trials (in mixed blocks) and single-task trials (in pure blocks).

While switching cost has received considerable attention, mixing cost has hardly been studied. The historical reason for this bias may be criticism regarding the original task-switching paradigms, which compared performance in pure-task blocks with that in mixed-tasks blocks (e.g., Jersild, 1927). According to the critics, the task-switch variable in these paradigms is confounded with some other variables that differentiate the two block types: working memory (WM) demands, division of attention between perceptual dimensions, degree of arousal and effort, response criterion, and so on (Meiran, 1996; Rogers & Monsell, 1995; Ward, 1982). However, recently, it has become increasingly evident that mixing cost may be at least as important as an indicator of executive control as switching cost. This is because mixing cost seems to capture the actual elicitation of executive control functions in response to the task instructions when the cognitive system is forced to operate under a complex and control demanding situation as compared with the simple and nearly automatic execution of a single task.

It is gradually being acknowledged that mixing cost and switching cost might reflect somewhat different control processes. Accordingly, mixing cost but not switching cost may reflect global control mechanisms or sustained control processes (Braver, Reynolds, & Donaldson, 2003; Koch, Prinz, & Allport, 2005; Kray & Lindenberger, 2000), whereas switching cost appears to be exclusively related to specific or transient control mechanisms (Braver et al., 2003; Logan & Bundesen, 2003; Mayr & Kliegl, 2003).

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One piece of evidence that mixing cost and switching cost represent different processes is the following behavioral double dissociation. Kray and Lindenberger (2000) found that whereas switching cost was relatively unaffected by old age, mixing cost was strongly affected by it (see also Mayr, 2001). However, Capeda, Capeda, and Kramer (2000) showed that children suffering from attention-deficit/hyperactivity disorder exhibit disproportionately large switching cost but normal mixing cost.

In light of these empirical dissociations between mixing cost and switching cost, and the much smaller volume of research directed on mixing cost, we decided to try and initiate a systematic exploration regarding the origins and the cognitive implications of mixing cost.

How can one distinguish, empirically, between factors that are related to mixing cost and those related to switching cost? We suggest the following scheme: A factor should be regarded as being related to mixing cost if it has different effects in mixed-tasks blocks as compared with pure blocks and has the same effect in switch and in repetition trials within the mixed blocks. A factor should be regarded as being related to switching cost if it has different effects on switch trials as compared with repetition trials within the mixed blocks.

Using this scheme, we can already show evidence in the literature that some factors might fit the definition of mixing cost and not that of switching cost. For example, it was found that explicit probabilistic task information provided to the participants in advance affects switch and repetition trials similarly (Ruthruff, Remington, & Johnston, 2001; Sohn & Carlson, 2000). Additionally, Meiran, Hommel, Bibi, and Lev (2002) found that the number of task alternatives has equal effect on switch and repetition trials. Although our interpretation of these findings as being related to mixing cost seems highly likely, none of the aforementioned studies included a single-task block, which makes it difficult to ascertain the unique role of these factors in mixing cost.

In the current work, we examine two hypotheses regarding potential sources of mixing cost in two independent experiments. Experiment 1 addressed the task-ambiguity hypothesis. This hypothesis refers to the notion that mixing cost reflects a transient process that is presumably triggered by the target stimuli in every mixed-tasks trial, switch and repetition trials included, but not within single-task blocks. Specifically, the hypothesized process refers to the resolution of task ambiguity resulting from the bottom–up activation of the competing task sets, caused by the fact that the same set of stimuli is used in both tasks. In Experiment 2, we addressed the hypothesis that mixed tasks conditions are characterized by increased WM load. We particularly tried to dissociate between two elements of WM load that have been frequently discussed in the literature: the storage of information (which is affected only by the number of task sets) and the manipulation of information (which is primarily affected by task ambiguity; see Baddeley, 1986; Baddeley & Logie, 1999).

To enable a comparison between the experiments, we used variants of the same paradigm in both of them. Participants switched between two main tasks: color discrimination and shape discrimination. These tasks were performed on the same set of stimuli: colored shapes (face, heart, clover, and musical notes) in four colors (red, blue, green, and yellow). To avoid potential confounding with practice, we used what we call a "sandwich-like" design in both experiments, in which the order of the conditions was as follows: single-task → mixed-tasks → single-task. In the mixed-tasks condition, the tasks were ordered randomly and were cued by instructional cues (De Jong, 1995a; Meiran, 1996; Shaffer, 1965). We manipulated task preparation time by varying the cue target interval (CTI) using two randomly determined CTIs: 100 ms (affording little or no preparation) and 1,000 ms (long preparation time).

In the extensive research on task switching, there is a robust finding that switch cost is reduced (and sometimes eliminated) when participants have sufficient time to prepare for a task switch (e.g., De Jong, 2000; Gopher, Armony, & Greenshpan, 2000; Meiran, 1996; Meiran et al., 2000; Rogers & Monsell, 1995). Some authors interpreted this finding as evidence for the involvement of internal control in the process of task switching and proposed various theoretical characterizations of the nature of that control. For example, Rogers and Monsell (1995) and Meiran (1996, 2000) proposed that a cognitive reconfiguration of the task rules takes place during CTI. Rubinstein, Meyer, and Evans (2001) and Sohn and Anderson (2001) defined the preparation process as a process of goal setting, and recently, Mayr and Kliegl (2000, 2003) suggested that the preparation for a task switch involves a cue-driven retrieval of task rules from long-term memory (LTM). Finally, Logan and Bundesen (2003) suggested that the reduction in switching cost reflects a perceptual process of cue repetition.

While all these theories attempt to explain the relation between preparation and switch cost, none of them specifically addresses mixing cost. However, it seems that different predictions regarding this relationship may be extracted from the different theories. Specifically, the theory of reconfiguration holds that the process of reconfiguration is required in switch trials and not in repetition trials. Therefore, on the basis of this theory, one would predict that preparation (CTI) would have little or no effect on mixing cost. In contrast, theories of goal setting suggest that this process is common to switch and repetition trials (especially Sohn & Anderson, 2001). In this case, one would predict a similar preparation effect on mixing cost and switching cost.

There are actually different findings that might support each of the predictions mentioned above. In Rogers and Monsell's (1995) experiments, the preparation interval did not affect performance of repetition trials, suggesting that preparation should not affect mixing cost. In contrast, Meiran et al. (2002) found that the number of task alternatives (conceptually related to the process of goal setting) affected switch and repetition trials but only in the short CTI condition, suggesting that preparation of task decision should take place both in switch and repetition trials and thus be related to mixing cost. Thus, in the following experiments, we hoped to shed more light on the relation between mixing cost and preparation by systematic manipulation of CTI.

Experiment 1: Task Ambiguity

In most task-switching experiments, the target stimuli being used in two (or more) tasks are overlapping, or *bivalent*. Namely, they have two (or more) dimensions, for example shape and color, each of which is relevant to one of the tasks. This type of bivalent stimuli has dual affordances and as a result can activate, in a bottom–up manner, the incorrect task and/or response in addition to activating the correct one and thus cause interference (e.g., Ruthruff et al., 2001). There is accumulating evidence that this feature of stimulus bivalence has a crucial influence on switching cost. In fact, even Jersild (1927), who was the first to conduct a systematic series of experiments on task switching, showed that when participants switched between two disjoint tasks, each being associated with a completely separate set of stimuli and responses, alternation cost was eliminated. This result was later replicated by Spector and Biederman (1976) and by Allport et al. (1994, Experiment 4). An even stronger result was recently found by Waszak, Hommel, and Allport (2003; cf. Allport & Wylie, 2000). These authors showed that a single trial was sufficient to link a specific stimulus (picture–word combination) with a specific task set (picture naming) and that later encounters with the same stimulus, which involved switching from that specific task to another (word reading), were accompanied by substantial slowing, over and above those associated with task switch itself.

These results suggest that the use of bivalent stimuli creates task ambiguity by the bottom–up cuing of the wrong task set. We predicted that this ambiguity could be especially influential in situations of task uncertainty. The term task uncertainty relates to the degree of which participants can anticipate the identity of the task in each upcoming trial. In single-task blocks, task uncertainty is 0% (participants have 100% confidence in the identity of the tasks in all trials). In randomly mixed-tasks blocks, task uncertainty is larger then 0% (it can be manipulated to different degrees by changing task probabilistic ratios, different task sequences, etc.). It has been implicated before that task uncertainty is related to mixing cost because it affects switch and repetition trials to the same degree (Dreisbach, Haider, & Kluwe, 2002; Meiran et al., 2002; Ruthruff et al., 2001). We therefore suggest that for a given degree of task uncertainty in the mixed blocks (which, in our experiments, was 50%), univalent stimuli reduce task uncertainty (because after the target stimulus is presented, it is 100% related to a specific task), whereas bivalent stimuli increase this uncertainty (by being related to both tasks). Thus, we propose that stimulus valence is also related to mixing cost.

Before proceeding to a systematic exploration of the contribution of stimulus valence to the two cost components, another important factor should be considered: When using bivalent stimuli, in addition to the potential bottom–up interference, participants also need to filter out irrelevant stimulus variation (in the sense discussed by Garner, 1970, 1974). Specifically, Garner showed that performance in a *single-task condition* is impaired when stimuli contain an irrelevant attribute that varies randomly across trials. On the basis of these results, he suggested the attentional selection hypothesis, according to which the need to filter an irrelevant dimension is in itself effortful and demands attentional resources. Note, however, that in Garner's (1970, 1974) studies, the irrelevant attribute was not related to any task that the participants performed. Thus, applying his findings to the current experimental context, the need to filter irrelevant information is presumably present in both the single-task condition and the mixed-tasks condition, and as such needs not contribute to mixing cost. However, we suggest that performing task in mixed blocks probably makes filtering especially difficult because, unlike in Garner's studies, the dimension to be ignored in a current trial is primed in the sense of having just been relevant in previous trials and having the potential to becoming relevant again in future trials. In many of the studies on task switching described above, participants performed each task under conditions of variation in an irrelevant feature. For example, in Allport et al.'s (1994) experiments, the task of reading words was performed under a condition in which the irrelevant ink color varied, and color naming was performed under conditions in which the irrelevant words varied. It is possible that at least some of the slowing in RT that was found in these studies can be attributed to the need to filter variation in the irrelevant but primed dimension.

These considerations lead us to distinguish between two important conditions: a condition with bivalent stimuli and a condition requiring filtering. A condition requires filtering if the value along the task-irrelevant dimension varies in the block. If that value is also one that appeared in the other task, then this condition is a bivalent one. However, if the value never appeared in the other task, then this condition is not bivalent because the stimulus could not be bound with that task (Waszak et al., 2003), and it could not prime a competing response. In our experiment, all target stimuli contained two dimensions (color and shape), each relevant for only one of the tasks. In the shape task, participants were required to determine whether the target was in the shape of a face or clover, and in the color task, they had to determine whether the target was red or blue. We created three conditions: In the first condition, univalent (Uni) without filtering, no filtering was required because for each task the target-irrelevant dimension was fixed (e.g., for the shape task, all target stimuli appeared in green color). In the second condition, univalent plus filtering (Uni + F), filtering was required, because for each task, the targets contained variance in the irrelevant dimension (e.g., for the shape task, target stimuli appeared in green or yellow; note that none of these colors were relevant under the color task). In the third condition, bivalent plus filtering (Biv + F), the target stimuli were bivalent, because for each task, the varied values of the irrelevant attribute were ones that appeared in the context of the other task (e.g., for the shape task, target stimuli appeared in red or blue—the same colors that were used for the color task).

Thus, in the following experiment, we independently manipulated stimulus valence and the need to filter an irrelevant feature, and we systematically measured the effects of these manipulations on mixing cost and switching cost. If the bottom–up interference from bivalent stimuli is crucial for mixing cost, as it probably is for switching cost, then one would expect mixing cost to be eliminated if the tasks are unambiguous. In contrast, if filtering of irrelevant dimension is a major factor in mixing cost (because, as we suggested, it might be more difficult to filter under the mixed-blocks condition), then one would expect mixing cost if there is a variation in the irrelevant dimension, even when the tasks are univalent.

Method

Participants

A total of 24 freshmen from either Ben-Gurion University of Negev (Beer-Sheva, Israel) or its affiliated college, Achva, took part in the experiment in return for partial course credit. All of them reported having normal or corrected-to-normal vision.

Apparatus and Stimuli

Stimuli were presented on an IBM personal computer clone with a 14-in. monitor (35.6 cm) and controlled by software written with the MEL 2.1 (Schneider, 1998) platform. Each trial began with a task cue in the form of an instructional word (the equivalent Hebrew words for "color" and "shape"), indicating which task should be performed in the current trial. The cue word (20 × 5 mm) was presented 2 cm above the center of the screen. The target stimuli were colored shapes (5 × 8 mm) presented at the center of the screen. Three types of targets were used, manipulated between groups:

1. In the Uni without filtering condition, the targets were univalent stimuli with a fixed irrelevant feature. Thus, in the shape task, the target could be face/clover always in green, whereas in the color task, the target could be red/blue heart. Note that in this condition, the irrelevant feature neither needed to be filtered nor could it be bound with and therefore prime the wrong task set.

2. In the Uni + F condition, the targets were univalent with a varying irrelevant feature. Thus, in the shape task, the target could be face/clover in green/yellow (green and yellow were not associated with the competing task but still needed to be filtered out), whereas in the color task, the target could be heart/musical notes in red/blue (heart and musical notes were not associated with the shape task). Note that in this condition, the irrelevant variant feature needed to be filtered but neither primed a competing response nor could it be bound with the wrong task.

3. In the Biv + F condition, all target stimuli were bivalent (face/clover in red/blue). For this target type, the need to filter was inherent, but the irrelevant feature also carried response-related and task-related information.

Participants responded with four keys of a regular keyboard, and responses were separated for the two tasks and grouped for each task to prevent response-related confusion. For example, in the color task, participants could use the two neighboring keys, "M" to indicate "red" and "N" to indicate "blue," and in the shape task, they pressed the neighboring keys "C" to indicate "face" and "X" to indicate "clover." The stimulus–response (S-R) mapping was counterbalanced between participants (see Figure 1 for a detailed description of the S-R mappings and the target stimuli in each group).

Procedure

The experiment was run as a single 1-hr session divided into five sections. At the beginning of each section, participants received written instructions specifying the task(s) to be performed in that section and the relevant S-R mappings. In the first section, participants performed a single-task block (either shape task or color task, counterbalanced across participants), including 48 trials preceded by 10 practice trials. In the second section, the participants performed a single-task block of the second task (either color task or shape task), including 48 trials preceded by 10 practice trials. In the third section, the mixed-blocks condition was introduced. Participants performed 15 practice trials and then eight blocks of mixed tasks, 48 trials each. The fourth and fifth sections were identical to the second and first sections, respectively. This sandwich-like design enabled the control of potentially confounding practice effect and created an equal number of single-task trials, repetition trials, and switch trials (see Figure 2 for a detailed description of the sandwich-like design). In all five sections, participants were instructed to be as quick and as accurate as possible. Within the blocks of trials, each trial began with the instructional task cue, followed by the presentation of the target after 100 or 1,000 ms (short or long CTI, respectively). The cue and target remained on the screen until the response was given. An incorrect response was followed by a 400 Hz beep presented for 100 ms. The interval from the n-1 response to the nth cue was fixed at 1,500 ms (see Figure 3 for trial sequence). In the single-task blocks, the 48 trials comprised all combinations of target (two in the Uni without filtering group and four in the other two groups) and CTI (two), replicated (12 or 6 times, respectively) in a random order. In the mixed-tasks blocks, the 48 trials comprised all combinations of task transition (two, repetition vs. switch), target (two or four), and CTI (two), replicated and presented in a random order.

Design

A between-participants independent variable was group (Biv + F, Uni + F, or Uni without filtering). The independent within-participants variables were (a) task transition (single task, repetition or switch) and (b) CTI (100 ms vs. 1,000 ms).

Results

The first trial of each block, trials associated with errors, and outliers (100 > RT > 3,000 ms) were discarded in all RT analyses (altogether, 1.3% of the trials were discarded).

A mixed analysis of variance (ANOVA) was carried out according to the design above. The means of this analysis are specified in Table 1. The group main effect was significant, $F(2, 21) = 6.17$, $p < .0077$, $MSE = 168{,}154.22$, indicating that participants in the Uni without filtering group were fastest (636 ms), followed by participants in the Uni + F (772 ms) group, and that participants in the Biv + F were slowest (930 ms). The task transition main effect was also significant, $F(2, 42) = 78.05$, $p < .0000$, $MSE = 13{,}180.66$, indicating that RT was fastest in single-task trials (640 ms), slowest in switch trials (932 ms), and intermediate in repetition trials (766 ms). A significant Group × Task Transition interaction was found, $F(2, 42) = 10.69$, $p < .0000$, $MSE = 13{,}180.66$. The pattern of this interaction is interesting (see Figure 4). The three groups did not differ significantly in the amount of switching cost according to an omnibus test as well as according to all the pairwise planned contrasts. The difference between switch and repetition trials was 167 ms in the Uni without filtering group, 210 ms in the Uni + F group, and 147 ms in the Biv + F group. However, a significant difference was found in the amount of mixing cost (the difference between repetition and single-task trials) between the groups. While mixing cost was large and significant in the Biv + F group (286 ms), $F(1, 21) = 47.42$, $p < .0000$, $MSE = 16{,}843.96$, it was minimal and nonsignificant in the Uni + F group (39 ms, $F < 1.00$) and in the Uni without filtering group (21 ms, $F < 1.00$). The CTI main effect was significant, $F(1, 21) = 132.22$, $p < .0000$, $MSE = 3{,}646.58$, and showed the expected pattern with faster responses after long CTI (721 ms) as compared with short CTI (837 ms). This variable also interacted significantly with group, $F(2, 21) = 8.67$, $p < .0018$, $MSE = 3{,}646.58$. The interaction indicated that the preparation (CTI) effect was smallest in the Uni without filtering group, intermediate in the Uni + F group, and largest in the Biv + F group (67 ms, 113 ms, and 169 ms, respectively). Finally, the triple interaction of Group × Task Transition × CTI was significant, $F(4, 42) = 4.87$, $p < .0026$, $MSE = 1{,}213.46$, indicating that the pattern of the Group × Task Transition interaction was especially pronounced in the short CTI. We note, however, that the pattern, although attenuated in the long CTI, was nevertheless similar to that seen in the short CTI. Namely, in both CTIs, mixing cost was significant in the Biv + F group and nonsignificant in the Uni + F and in the Uni without filtering groups.

We also computed planned contrasts to explore the effect of the filtering manipulation. First, RT level was, as expected, higher in

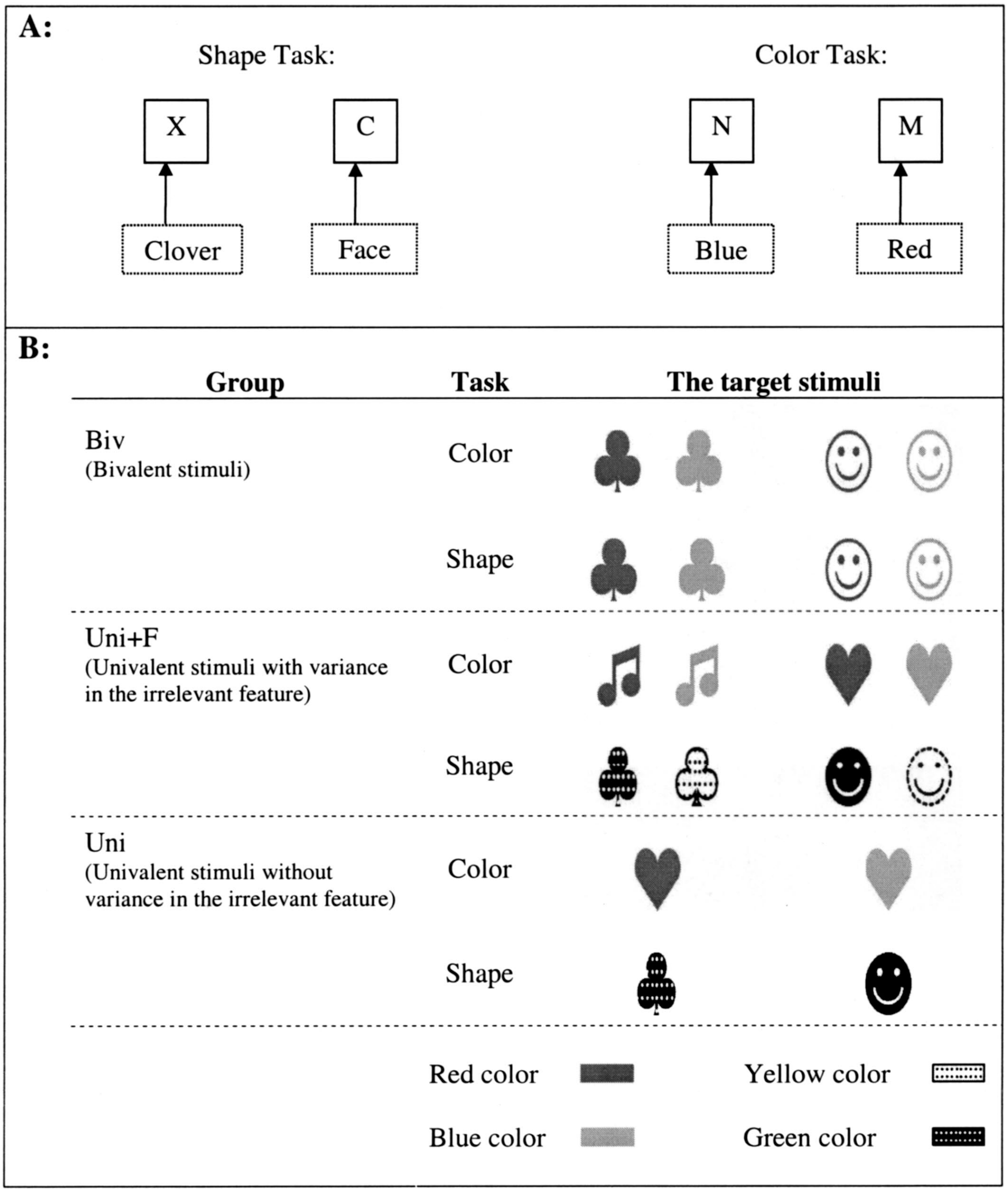

Figure 1. (A) An example of the stimulus–response (S-R) mappings in the shape task and color task in Experiment 1 (half of the participants used this S-R mapping and the other half used a reversed mapping). (B) The different target stimuli used in Experiment 1 as a function of task (color or shape) and group (bivalent [Biv], univalent plus filtering [Uni + F], or univalent [Uni] without filtering). Note that the colors of the stimuli are represented by different shades and fillings (see legends at the bottom).

the Uni + F relative to the Uni without filtering group (772 vs. 636 ms). This difference approached significance, $F(2, 21) = 2.67$, $p < .06$, $MSE = 168{,}154.22$. Additionally, when comparing mixing cost and switching cost in long and short CTI between Uni + F and Uni without filtering groups, only one contrast was significant: Under short CTI, the switching cost was significantly higher in the Uni + F group (270 ms) than in the Uni without filtering group (172 ms), $F(1, 21) = 3.67$, $p < .0490$, $MSE = 5{,}240.45$.

Block type	Color task	Shape task	Mixed tasks	Shape task	Color task
Number of trials	10 + 48	10 + 48	15 + 8x48	10 + 48	10 + 48

Figure 2. The “sandwich-like” design of block sequence and number of trials in each block in Experiment 1.

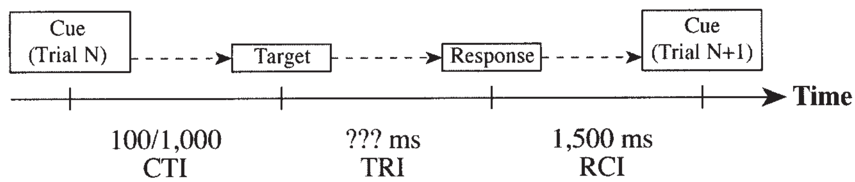

Figure 3. The trial sequence in Experiments 1 and 2. CTI = cue target interval; TRI = target response interval; RCI = response cue interval.

Proportion of errors (PE; M = .03) was submitted to the same mixed ANOVA as above. The main effect of task transition was significant, $F(2, 42) = 17.71$, $p < .0000$, $MSE = 0.00035$. PE was .04 in the switch condition, .02 in the repetition condition, and .025 in the single-task condition. At a first look, it seems that the direction of the difference between repetition and single task is reversed in relation to the direction of that difference in RT (which might indicate a speed–accuracy trade-off). However, a planned contrast indicated that this difference was not significant, $F(2, 21) = 1.81$, $p < .19$, $MSE = 0.00038$. Thus, only PE in the switch condition was significantly higher than PE in the two other conditions, $F(2, 21) = 36.73$, $p < .0000$, $MSE = 0.00032$. The effect of CTI was also significant, $F(1, 21) = 5.17$, $p < .0335$, $MSE = 0.00036$, and the trend was in the same direction as the RT effect. PE was higher in the short CTI condition (.03) relative to the long CTI condition (.02). The Task Transition × CTI interaction approached significance, $F(2, 42) = 2.45$, $p < .1$, $MSE = 0.00028$, with a pattern similar to that seen for RT. All other main effects and interactions were nonsignificant (all Fs < 1.00 or very close to 1.00). Thus, our conclusions above are not compromised by any observable speed–accuracy trade-off.

Discussion

In this experiment, three groups of participants differed in the type of target stimuli. One group of participants reacted to univalent stimuli, which did not vary along the irrelevant dimension (the Uni without filtering group). In this group, presumably, no bottom–up activation of the task occurred, and no filtering of an irrelevant stimulus dimension was required. A second group also reacted to univalent stimuli. However, these stimuli varied in the irrelevant feature (Uni + F group). In this group, presumably, no bottom–up interference occurred, but participants needed to filter the irrelevant feature of the stimuli. A third group of participants reacted to bivalent stimuli (stimuli that contain features that are relevant to both tasks). In this group only, the stimulus could be bound with the wrong task and could therefore cue that task in a bottom–up manner and interfere with performance.

The results indicate that the valence manipulation affected mixing cost and not switching cost. Specifically, large mixing costs were found in the bivalent group, and these were eliminated in the two groups that reacted to univalent stimuli. In contrast to mixing cost, there was no significant difference in the amount of switching cost between the three groups. The general pattern of these results indicates that stimulus valence corresponds to our definition of factors that are related to mixing cost but not to switching cost: It had a differential effect on single-task blocks and mixed blocks and no differential effect on switch and repetition trials within the mixed blocks.

As described in the Introduction section, several studies have already manipulated stimulus ambiguity in the task-switching paradigm. However, almost none of them measured the effect of this manipulation on mixing cost. We found only one piece of evidence in the literature that converges with our findings. Specifically, Mayr (2001) found that mixing cost (which he calls “global set selection cost”) was substantial only when stimuli were ambiguous and response specifications overlapped for the relevant tasks. In his experiments as well, mixing cost was significant even after long preparation time.

Table 1
Mean RT (Standard Error), Mixing Cost, and Switching Cost in Milliseconds as a Function of Group (Biv Plus Filtering, Uni Plus Filtering, or Uni Without Filtering), Task Transition (Single Task, Repetition, or Switch), and CTI (100 ms vs. 1,000 ms) in Experiment 1

Group	Task transition			Mixing cost	Switching cost
	Single task	Repetition	Switch		
Biv plus filtering					
Short CTI	690 (46)	1,082 (67)	1,270 (74)	382	188
Long CTI	649 (54)	889 (66)	997 (76)	240	108
Uni plus filtering					
Short CTI	682 (46)	767 (67)	1,037 (74)	85	270
Long CTI	670 (54)	665 (66)	815 (76)	−5	150
Uni (without filtering)					
Short CTI	584 (45)	625 (67)	797 (74)	41	172
Long CTI	565 (54)	568 (66)	675 (76)	3	107

Note. RT = reaction time; Biv = bivalent; Uni = univalent; CTI = cue target interval.

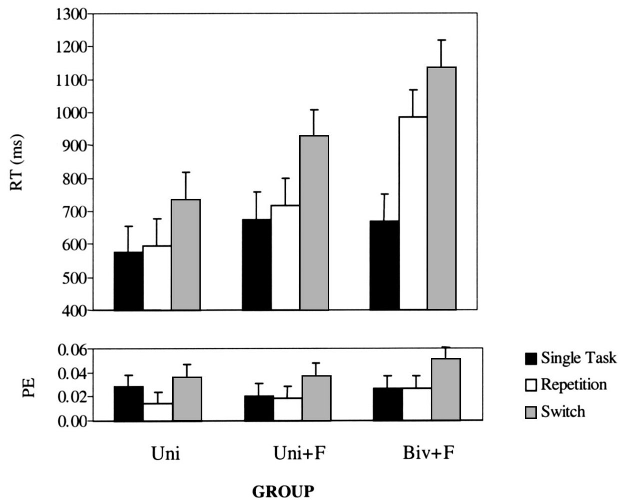

Figure 4. Reaction time (RT) in milliseconds and proportion of errors (PE) as a function of task transition (single task, repetition, or switch) and group (bivalent plus filtering [Biv + F], univalent plus filtering [Uni + F], or univalent [Uni] without filtering) in Experiment 1. The thin bars represent the 95% confidence interval.

What might be the mechanism that mediates the effect of stimulus valence on mixing cost? It is hypothesized that bivalent stimuli can activate, in a bottom–up manner, the competing task set or response. Why does this competition influence performance in mixed blocks and not in single-task blocks? Additionally, why does this competition have the same influence in switch and repetition trials? To answer these questions, one must look for a process that is exclusive to mixed blocks and common to switch and repetition trials. We propose that this process is the *task-decision* process (also related to as *goal-setting* process) hypothesized by Fagot (1994), Rubinstein et al. (2001), and Sohn and Anderson (2001). It was suggested that in the task-cuing paradigm, when the order of tasks is random, a task decision process takes place in each trial within the mixed blocks. This process is susceptible to interference from a bottom–up activation caused by the target stimulus. Specifically, if the stimulus activates the competing task set, then this competition must be resolved for the relevant task decision to be accomplished.

It could be argued that the valence effect on mixing cost is not related to the control process of task decision but to a lower, more specific process of rule implementation (e.g., Mayr & Kliegl, 2003) or response selection (e.g., Meiran, 2000). However, if this was the case, then one would expect a stronger effect of valence in switch trials than in repetition trials. The reason is that, in switch trials, the activation of the competing rule comes from two sources: the preceding trial and automatic retrieval of the wrong rule due to the bivalence of the stimulus. In contrast, in repetition trials, competition comes only from the current bivalent target, and even more, the activation due to the preceding trial is of the correct rule rather than the competing rule. Our findings, however, show an equivalent valence effect in switch and repetition trials. In addition, we believe that the interaction between CTI, task transition, and group in the experiment also supports our line of interpretation regarding the process of task decision. Specifically, although the pattern of group effect on mixing cost (represented by large mixing cost in the bivalent group and an absence of mixing cost in the univalent groups) was observed in both CTIs, it was more pronounced when the CTI was short. The reduced valence effect after long preparation implies that when there was enough time to complete the task-decision process in advance, and the relevant task set was highly activated before target presentation, the effect of the competing set activated by the stimulus was smaller. The current conclusion is in line with that of a number of recent articles, all showing that preparation overcomes bottom–up influences (Koch & Allport, in press; Meiran & Daichman, in press; Rubin & Koch, in press; Yeung & Monsell, 2003).

In several previous studies, a significant reduction in switching cost was found when the stimuli were univalent (Allport et al., 1994; Meiran, 2000; Rogers & Monsell, 1995; Spector & Biederman, 1976). These results contrast with our results that showed no effect of stimulus valence on the amount of switching cost. A potential explanation for this discrepancy is our use of a setup with disjoint responses in the two tasks (the so-called univalent response setup; see Meiran, 2000), which contrasts with the use of overlapping responses (the so-called bivalent setup) in these other studies.

Allport and Wylie (2000) and Waszak et al. (2003) also presented findings in contradiction with our results. These researchers found that, unlike in our experiment, stimulus-based task cuing (manipulated by introducing the same stimuli in the context of two different tasks) affected switching cost. This contradiction can be potentially explained by the different paradigms that were used in the experiments. Both Allport and Wylie and Waszak et al. used the alternating-runs task-switching paradigm (see Rogers & Monsell, 1995). As explained in previous paragraphs, we assume that the stimulus-based interference affects the process of goal setting or task decision. While this process takes place in every trial in the cuing task-switching paradigm (that we used), it is only needed in the first trial of every run in the alternating-runs paradigm, which is a switch trial. Therefore, the same mechanism through which stimulus-based interference occurs can cause an increase of switching cost in the alternating-runs paradigm and an increase of mixing cost in the cuing task-switching paradigm.

To conclude, we suggest that the influence of stimulus valence is crucial for mixing cost and is potentially mediated by the task-decision mechanism. Although we did not find an effect of stimulus valence on switching cost, this effect has been shown to be extensive in other studies. Still, it should be emphasized that significant switch cost occurs also in the absence of stimulus ambiguity (bivalence). In addition, it was shown that the need to filter the irrelevant dimension of the stimuli does not result in enhanced mixing cost. If at all, filtering causes an increase in switching cost but only when no time for advanced task preparation is provided.

Experiment 2: WM Load

Los (1996) suggested that single-task and mixed-tasks blocks differ in "mental load": The load is higher in mixed blocks "due to the mere requirement to maintain readiness of all mental structures that could be called upon by either level of the independent variable" (p. 183). The most common conceptualization of this requirement is in terms of WM load (i.e., Baddeley, 1986; Baddeley & Logie, 1999). In the task-switching literature, Rogers and

Monsell (1995) have also speculated that one crucial difference between mixed-tasks and single-task condition is WM load.

Only a few studies have examined the role of WM load in mixed-tasks condition. Baddeley, Chincota, and Adlam (2001) demonstrated the effect of extra WM demands on performance of simple tasks under a task-alternation condition. They found that introducing a demanding secondary task increases task-alternation cost. Unfortunately, their study did not include repetition trials within the mixed-blocks condition, so it is impossible to conclude whether WM load affected switching cost, mixing cost, or both. Another study, performed by Emerson and Miyake (2003, Experiment 4), demonstrated that manipulating the number of tasks within the block did not affect switch cost. In their study, however, number of tasks was confounded by task ambiguity. In addition, they did not use a single-task condition and, thus, could not differentiate between switching cost and mixing cost.[1]

The important question is then whether it is the simple addition of S-R rules to WM that contributes to mixing cost. In Experiment 2, we therefore tried to evaluate the exclusive effect of additional S-R rules on mixing cost and switching cost. To load WM in a manner that would not also increase task ambiguity, as studied in Experiment 1, we used four tasks from two categories: (a) the two object tasks that were used in Experiment 1—shape/color discrimination tasks; (b) two spatial tasks (based on Meiran's, 1996, study), in which a target stimulus is presented in one of four positions of a 2 × 2 grid, and participants have to decide whether it is in the upper or lower part of the grid (up–down task) or whether it is in the right or left part of the grid (right–left task). Within each task category, stimuli and responses were bivalent, but between categories, there was no overlapping of stimuli or responses whatsoever. Four conditions were compared in a within-subjects experiment:

1. Single-task condition—pure blocks of each of the four tasks.

2. Mixed between categories (MBC) condition—mixed blocks of two tasks from different categories (e.g., the shape task and the up–down task). In this condition, there is additional rules load without additional ambiguity load.

3. Mixed within category (MWC) condition—mixed blocks of two tasks from the same category, namely, either the two spatial tasks or the two object tasks. In this condition, there is additional rules load together with additional ambiguity load.

4. Mixed within and between categories (MWBC) condition—mixed blocks of three tasks: two from the same category and one from the other category. For example, the two object tasks mixed with one spatial task. In this condition, the effect of extra WM load (3 vs. 2 tasks) was measured both in the case of the need to execute one of the ambiguous tasks (the object tasks in the example) and in the case of the need to execute the unambiguous task (the spatial task in the example).

Thus, two subconditions were realized: an ambiguous–MWBC (AMWBC) condition and an unambiguous–MWBC (UMWBC) condition. When referring to AMWBC condition, we refer to the two tasks among the three tasks, which were performed on an overlapping set of stimuli and used overlapping responses. Specifically, we refer to the color and shape tasks, when both of them were involved in the block together with an additional spatial task. When referring to UMWBC condition, we refer to the task that was performed on a separate set of stimuli and used different responses as the other two tasks. Specifically, we refer to conditions in which either shape or color but not both were performed together with the two spatial tasks.

Method

Participants

Eight students took part in the experiment in return for partial course credit. They had similar characteristics as those who took part in Experiment 1.

Apparatus and Stimuli

The monitor and the presentation of the object tasks and task cues were identical to the ones in Experiment 1. In the spatial tasks, the cue appeared together with a 2 × 2 grid (in which the target stimulus—a small "smiley"—appeared later) and comprised two small arrows located either above and below the grid (to indicate up–down task) or in the right and left sides of the grid (to indicate right–left task; see Figure 5A). The object tasks were basically identical to the ones used in Experiment 1 except for one difference: The face target was replaced by a heart to avoid similarity with the target stimulus of the spatial tasks. Thus, the target stimuli in the object tasks were a heart or clover in red or blue. Participants responded with the first and middle fingers from both hands on four keys of a regular keyboard: For the objects tasks, participants pressed "C" to indicate "blue" under the color task and "clover" under the shape task, and they pressed "I" to indicate "red" under the color task and "heart" under the shape task. For the spatial tasks, participants pressed "V" (located in the lower left position) to indicate "down" under the up–down task and "left" under the right–left task, and they pressed "U" (located in the upper right position) to indicate "up" under the up–down task and "right" under the right–left task (see Figure 5B).

Procedure

The experiment was run in three 1-hr sessions. The first session was divided into three sections. In Section 1, participants performed four single-task blocks (single-task condition), one for each of the four tasks presented throughout the experiment. Before each block, written instructions regarding the S-R rules of that task were given. Each block contained 48 trials preceded by 10 practice trials. The order of categories (object or spatial tasks) was counterbalanced across participants and so was the order

[1] We wish to note a potential WM effect that is paradigm-related. In the alternating runs paradigm (e.g., Rogers & Monsell, 1995), the order of tasks within a block is constant and is revealed to the participants in advance and thus needs to be held in some form of memory. In contrast, in the cuing task-switching paradigm (e.g., Meiran, 1996), the order of tasks is randomly determined, and the identity of the relevant task is indicated by a task cue. Indeed, in Baddeley et al.'s (2001) experiments, which used fixed task sequence, adding a secondary task increased alternation cost to a lesser degree when redundant task cues were presented so that the participants did not need to hold the task sequence in memory. Nonetheless, WM load increased the alternation cost even when the redundant task cues were present.

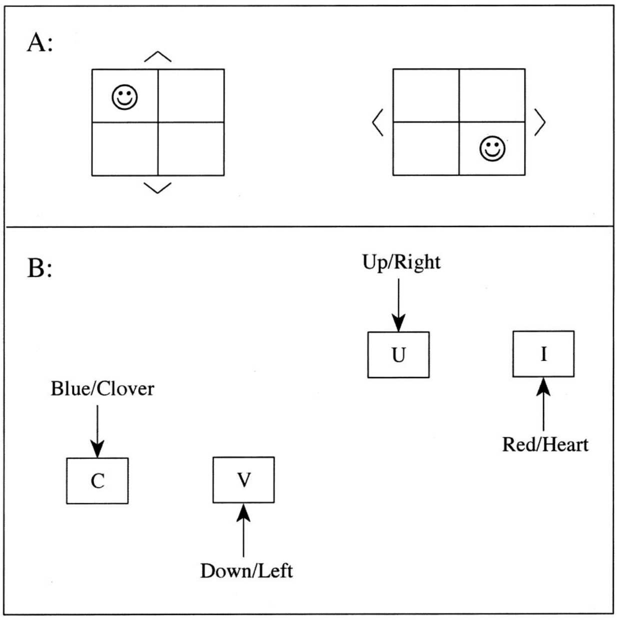

Figure 5. (A) An example of the 2 × 2 grid, the arrow task cues, and target stimulus in the spatial tasks (up–down or right–left) that were used in addition to the object tasks (shape and color) in Experiment 2. (B) An example of the stimulus–response mappings in the shape task, color task, up–down task, and right–left task in Experiment 2. Note that the responses are overlapping within task category and nonoverlapping between the tasks categories.

of tasks within each category; for example, the order of blocks in the section could be as follows: shape task → color task → up–down task → right–left task). In Section 2, participants performed four blocks (48 trials each preceded by 10 practice trials) of between-categories mixed tasks (MBC condition). At the beginning of each block, written instructions were given regarding the S-R rules of the two relevant tasks. There were four possible between-categories combinations (shape + up–down, shape + right–left, color + up–down, color + right–left). Each participant performed two blocks of two combinations (e.g., two blocks of shape + up–down tasks and two blocks of color + right–left tasks). Combinations and order of combinations were counterbalanced across participants. In Section 3, participants performed four blocks (48 trials each preceded by 10 practice trials) of within-category mixed tasks (MWC condition). Again, written instructions were given before each block. Each participant performed two blocks of mixed tasks from the object-tasks category (shape and color) and two blocks from the spatial-tasks category (up–down and right–left). The order of categories was counterbalanced across participants. At the beginning of the second session of the experiment, participants received a written reminder of the four tasks and their S-R rules, and then they performed four practice blocks of a single task (15 trials each). Then participants performed eight blocks (48 trials each) of three mixed tasks between and within categories (MWBC condition). Again, there were four possible combinations of tasks: two object tasks + up–down, two object tasks + right–left, two spatial tasks + color, and two spatial tasks + shape. Each participant performed four blocks from two of the possible categories (again, combinations and order of combinations were counterbalanced across participants). The third session was identical to the first session but the order of blocks was reversed (participants performed four blocks of MWC condition, four blocks of MBC condition, and four blocks of single-task condition). As in Experiment 1, this sandwich-like design enabled the control of potentially confounding practice effect and created an equal number of trials from each category throughout the experiment. All the other details were identical to the ones in Experiment 1 (CTI manipulation, trials sequence, etc.).

Design

The independent within-participants variables were as follows:

1. Task transition (nine levels were defined): single task, repetition/switch in blocks of MBC condition (R-MBC/S-MBC), repetition/switch in blocks of MWC condition (R-MWC/S-MWC), repetition/switch of the task from the ambiguous category in blocks of MWBC condition (R-AMWBC/S-AMWBC), and repetition/switch of the task from the unambiguous category (third task) in blocks of MWBC condition (R-UMWBC/S-UMWBC).

2. CTI (100 ms vs. 1,000 ms).

Results

In the current article, we only discuss the data from trials of the object tasks, because these are parallel to the tasks in Experiment 1. As a general note regarding the spatial tasks, we can say that they yielded quicker responses, but that the performance profile across the conditions was the same as that found for the object tasks, as indicated by the lack of any significant interactions between task–category and the other variables, in a preliminary analysis. The first trial of each block, trials associated with errors, and outliers ($100 > RT > 3{,}000$ ms) were discarded in all RT analyses (altogether 5.4% of the trials).

A repeated measures ANOVA was carried out on all trials of the object tasks according to the design specified above. The means for this analysis are specified in Table 2.

Although we focused on the planned contrasts to answer our questions, we also report the ANOVA results. The main effects of task transition and CTI (RT short = 838 ms, RT long = 656 ms) were significant, $F(8, 56) = 10.25$, $p < .0000$, $MSE = 18{,}371.00$; $F(1, 7) = 128.09$, $p < .0000$, $MSE = 9{,}236.85$, respectively. The Task Transition × CTI interaction was also significant, $F(8, 56) = 7.79$, $p < .0000$, $MSE = 4{,}358.20$. To answer our more focused questions, we performed a series of planned contrasts. These contrasts were grouped into two groups: contrasts on mixing cost and contrasts on switching cost. Within each group, four questions were addressed. For brevity, we refer to accuracy data only when there was a numerical trend suggesting a speed–accuracy trade-off.

Contrasts Related to Mixing Cost

Because mixing cost is the difference in performance between repetition trials and single-task trials, we computed contrasts for repetition trials in the different conditions (see Figure 6) to answer the following four questions:

1. Did mixing of two tasks from different categories produce mixing cost? Surprisingly, it did. The difference between R-MBC and single task (94 ms) was significant, $F(1, 7) = 6.33$, $p < .0400$, $MSE = 11{,}164.37$. When introducing the CTI variable to the contrasts, it was revealed that the mixing cost was significant under short CTI, $F(1, 7) = 8.61$, $p < .0215$, $MSE = 5{,}983.50$, but not under long CTI, $F(1, 7) = 2.99$, $p < .13$, $MSE = 7{,}327.97$.

2. Did mixing of two tasks from the same category produce mixing cost? Unsurprisingly, it did. The difference between R-MWC and single task (204 ms) was significant, $F(1, 7) = 65.30$, $p < .0000$, $MSE = 5{,}151.39$. Although this effect was significant

Table 2
Mean RT (Standard Error), Mixing Cost, and Switching Cost in Milliseconds as a Function of Task Transition (Single-Task, R-MBC, S-MBC, R-MWC, S-MWC, R-AMWBC, S-AMWBC, R-UMWBC, and S-UMWBC) and CTI (100 ms vs. 1,000 ms) in Experiment 2

	CTI	
Group	Short (100 ms)	Long (1,000 ms)
Single-task blocks	586 (33)	547 (23)
Mixed between category (MBC)		
Repetition–MBC (R-MBC)	700 (43)	621 (49)
Switch–MBC (S-MBC)	879 (85)	712 (63)
Mixing costs	114	74
Switching costs	179	91
Mixed within category (MWC)		
Repetition–MWC (R-MWC)	875 (41)	668 (57)
Switch–MWC (S-MWC)	1,004 (45)	725 (61)
Mixing costs	289	121
Switching costs	129	57
Mixed within and between categories (MWBC)—Trials from the ambiguous category		
Repetition–ambiguous–MWBC (R-AMWBC)	905 (73)	712 (83)
Switch–ambiguous–MWBC (S-AMWBC)	1,054 (90)	713 (74)
Mixing costs	319	165
Switching costs	149	1
Mixed within and between categories (MWBC)—Trials from the unambiguous category		
Repetition–unambiguous–MWBC (R-UMWBC)	711 (45)	538 (30)
Switch–unambiguous–MWBC (S-UMWBC)	826 (44)	671 (60)
Mixing costs	125	−9
Switching costs	115	133

Note. RT = reaction time.

under both CTIs, it was still significantly larger under short CTI than under long CTI (289 vs. 121 ms), $F(1, 7) = 11.97$, $p < .0105$, $MSE = 4{,}722.88$.

The accuracy data for this contrast under long CTI condition had an opposite pattern. It indicated negative mixing cost: PE was lower in R-MWC relative to single task (.027 vs. .032). However, a post hoc contrast revealed that this difference was not significant ($F < 1.00$).

3. Did task ambiguity affect performance in repetition trials? It did. RT in repetition trials was significantly shorter when two tasks from different categories were mixed (R-MBC) than when two tasks from the same category were mixed (R-MWC; 661 vs. 772 ms), $F(1, 7) = 7.38$, $p < .0299$, $MSE = 13{,}378.79$. In these two conditions, the number of rules to be held in WM was equal, but in the R-MWC condition, there was an additional task ambiguity related to the fact that the two tasks were performed on the same set of stimuli and used the same response keys. Thus, it can be concluded that task ambiguity affects mixing cost independently from additional WM load, a conclusion that is in line with what we found in Experiment 1. The effect was stronger under short CTI than under long CTI, $F(1, 7) = 9.33$, $p < .0184$, $MSE = 3{,}528.72$.

The pattern of PE data in this contrast was opposite to the pattern of RT, which might indicate speed–accuracy trade-off. Specifically, PE in the R-MBC condition was higher then in the R-MWC condition (.045 vs. .036). This pattern, however, was not significant, $F(1, 7) = 2.58$, $p < .15$, $MSE = 0.00028$.

4. Did loading WM with additional S-R rules affect performance in repetition trials? Surprisingly, the answer to this question is negative. There was no significant difference in RT between R-MWC trials (repetition trials in which two ambiguous tasks were mixed) and R-AMWBC trials (repetition trials of one of the ambiguous tasks in blocks of three mixed tasks; $F < 1.00$ under both CTIs). The difference between these two conditions was only in the number of S-R rules to be held in WM. The reason is that, in both conditions, the task was performed on the same set of stimuli as another task in that block and used the same responses. The difference is that in the AMWBC condition, there was an additional nonambiguous task performed in the block.

The same question was addressed in another contrast comparing R-UMWBC trials (repetition of the unambiguous [third] task in blocks of three mixed tasks) and R-MBC trials (repetition trials in blocks of two unambiguous tasks). This contrast also turned out nonsignificant, $F(1, 7) = 1.49$, $p < .27$, $MSE = 6{,}986.59$. The difference between these two conditions is also only in the number of rules to be loaded in WM. In both conditions, the task was unambiguous, but it was performed in context of two more tasks in the R-UMWBC condition and performed in a context of only one more task in the R-MBC condition. Together, the results in this section indicate that loading WM with additional S-R rules in itself does not increase mixing costs.

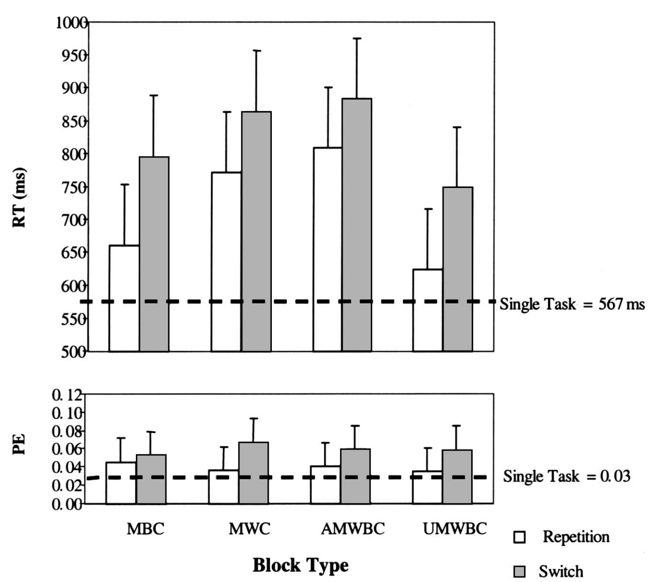

Figure 6. Reaction time (RT) in milliseconds and proportion of errors (PE) as a function of task transition (repetition or switch) and block type (mixed between category [MBC], mixed within category [MWC], ambiguous–mixed within and between categories [AMWBC], or unambiguous–mixed within and between categories [UMWBC]) in Experiment 2. The data of the single-task condition are marked by the horizontal striped line. The thin bars represent the 95% confidence interval.

Contrasts Related to Switching Cost

Switching cost is measured by the difference between switch and repetition trials within blocks of mixed tasks (see Figure 6). Accordingly, we performed a series of planned contrasts to answer the following four questions.

1. Did mixing of two tasks from different categories produce switching cost? It did. The difference between S-MBC and R-MBC (129 ms) was significant, $F(1, 7) = 14.45$, $p < .0067$, $MSE = 10{,}077.94$. There was no significant difference in the size of this effect between short and long CTI. This result is compatible with the results of Experiment 1, in which switching cost was found in spite of the fact that the targets were univalent (unambiguous).

2. Did mixing of two tasks from the same category produce switching cost? Unsurprisingly, it did. The difference between S-MWC and R-MWC (93 ms) was significant, $F(1, 7) = 15.14$, $p < .0059$, $MSE = 4{,}504.93$. Surprisingly, there was no significant difference in the size of this effect between short and long CTI.

3. Did ambiguity affect performance in switch trials? No, it did not. There was no significant difference in RT between S-MBC trials and S-MWC trials, $F(1, 7) = 1.27$, $p < .30$, $MSE = 29{,}559.59$. In these two conditions, the number of rules to be held in WM was equal, but in the S-MWC condition, there was an additional task ambiguity. Thus, it can be concluded that task ambiguity does not significantly affect performance beyond the switch effect.

4. Did loading WM with additional S-R rules affect performance in switch trials? No, it did not. In the relevant contrast, we found that there was no difference in RT between S-MWC trials (switch trials in which two ambiguous tasks were mixed) and S-AMWBC (switch trials of one of the ambiguous tasks in blocks of three mixed tasks; $F < 1.00$ under both CTIs). The difference between these two conditions was only in the number of rules to be held in WM (in both conditions there are the same two ambiguous tasks, and in AMWBC there was an additional unambiguous task in the block). This conclusion was further corroborated in another contrast in which we found no significant difference in RT between S-UMWBC trials (switch to the unambiguous [third] task in blocks of three mixed tasks) and S-MBC trials (switch trials in blocks of two unambiguous tasks), $F(1, 7) = 2.04$, $p < .20$, $MSE = 8{,}763.66$. The difference between these two conditions was also only in the number of rules to be held in WM. In both conditions, the task performed was unambiguous, but it is in context of two more tasks in the S-UMWBC condition and only one more task in the S-MBC condition. Together, the results in this section indicate that additional WM load in itself does not increase switch RT.

The mean PE in trials of the object tasks category was .045. As mentioned, the PE data were submitted to the same analysis as the RT data, and in the paragraphs above, we reported all cases that could be interpreted as evidence for speed–accuracy trade-off. In none of these cases did the problematic pattern of PE data come out significant. Thus, there was no evidence that speed–accuracy trade-off could have compromised any of our conclusions.

Discussion

In the current experiment, the tasks were executed in five different contexts: (a) single-task blocks, (b) blocks in which one of the object tasks was mixed with a task from a completely nonoverlapping category (spatial task), (c) blocks in which the two objects tasks were mixed, (d) blocks in which the two object tasks were mixed together with a third task from a completely nonoverlapping category (spatial task), and (e) blocks in which one of the object tasks was mixed with two other tasks (spatial tasks) that were overlapping with one another but not with the object task. In these contexts, repetition and switch trials were performed after short or long preparation intervals. This complex combination of conditions enabled us to disentangle the effect of additional WM load (in terms of number of rules to be held and retained) from the effect of additional task ambiguity. We found that while task ambiguity dramatically affected mixing cost, it had no effect on switching cost, thus converging with the results of Experiment 1—the simple addition of task sets or rules to WM load neither affected mixing cost nor did it affect switching cost.

The WM-Load Hypothesis.

It was commonly suggested that the additional load in WM, caused by the fact that in mixed blocks more task rules must be held in WM storage, reflects a sustained control process that contributes to the creation of mixing cost (e.g., Braver et al., 2003; Los, 1996). To test this hypothesis, we compared equivalent conditions that differed only in the number of relevant S-R rules in the given block of trials. Adding a third unambiguous task to a pair of ambiguous tasks did not affect RT in these tasks. Similarly, adding two ambiguous tasks or just one task to an unambiguous task also did not affect RT. The only exception was one in which we compared single task with a condition in which participants switched between two unambiguous tasks. There, we observed slowing. It might be argued that this pattern of results implies that WM load affects mixing cost in a stepwise manner, namely, that only the step from one to two task sets is meaningful in creating mixing costs. We have two reasons to believe that this is not the case: (a) In Experiment 1, the equivalent condition (i.e., mixing of two univalent tasks) produced no mixing cost, suggesting that the increase of load from one task to two tasks per se does not produce mixing cost, and (b) We found an interesting (even surprising) result regarding the condition of three mixed tasks. In that condition, when participants performed the nonoverlapping task after long preparation time, they had no significant mixing cost. Thus, there is at least one exemplar condition in which no mixing cost occurred despite the fact that more than one task set is involved within the block.[2] We therefore find it appropriate to conclude that the WM-load hypothesis for the explanation of mixing cost is not valid, at least in the paradigm we used. We suggest an explanation for the slowing in the MBC condition below.

[2] The finding that mixing cost occurred when two nonoverlapping tasks were mixed and not when the nonoverlapping task was mixed with two more tasks is perplexing. We thank Ulrich Mayr for suggesting the following explanation to explain this apparent anomaly: In the nonoverlapping condition (MBC), there is no need to use task cues because all stimuli are nonambiguous. However, in the UMWBC condition, cue processing is necessary and knowing what comes next may be actually helpful, even when stimuli are nonambiguous. In other words, the difference in costs between the two conditions may be a result of differences in cue-related processes. This explanation is compatible also with our other theoretical interpretations for the results.

In fact, our results are compatible with some indirect evidence in the literature that implicates the lack of relation between simple WM load and mixing cost or even switching cost. Baddeley et al. (2001) demonstrated that loading WM with an additional task, completely different from the adding/subtracting tasks that participants were required to perform, interfered with performance in mixed blocks but not in single-task blocks. However, this result was valid only when the additional task was one that occupied the phonological loop subsystem. When the additional load was on the visiospatial sketchpad, a slight general slowing of RT was observed, but it was common to single-task and mixed-tasks blocks (for a review of the elaborated model of the central executive and its subsystems, see Baddeley, 1986; Baddeley & Logie, 1999). Thus, these authors also showed that an additional load of task rules is not in itself sufficient to produce mixing cost or switching cost. Another piece of evidence that supports our findings comes from the clinical literature. Keele and Rafal (2000) assessed switching cost and mixing cost in patients suffering from prefrontal lesions. As a control to the set-switching condition, the researchers used a situation in which the same number of S-R rules was required (i.e., eliminating the S-R rule load confound). A very large mixing cost was obtained that was limited to left-frontal patients, whereas switching cost did not differ across groups. Thus, at least in particular patient groups, it is possible to find mixing cost even when WM load plays no obvious role.

Our results are also in line with an argument made by Mayr and Kliegl (2000, 2003). These authors claimed that it is actually not the case that two different task sets could be simultaneously held in WM. They proposed that during the performance of mixed blocks, all task sets are stored in the LTM and that switching between tasks requires a process of LTM retrieval. In that case, the number of task sets that are mixed within a block does not affect the difficulty of their retrieval (because the capacity of LTM is not limited). Our results support this view to some degree by showing that there is no relationship between the number of task sets and the mixing and switching costs. Mayr and Kliegl proposed that other factors, such as the type of information to be retrieved (e.g., episodic vs. semantic), influence switching cost.

We would like to note a confound between our WM manipulation and another potential source for mixing cost that is not discussed in the current article. In Experiment 2, WM load was confounded with *task uncertainty*. The term task uncertainty (or its complementary—*task expectancy*) relates to the degree that participants can predict in advance the identity of the task in future trials throughout the block (e.g., Dreisbach et al., 2002; Ruthruff et al., 2001; Sohn & Anderson, 2001). Obviously, in the paradigm we used, single-task blocks offer 100% task expectancy, whereas in randomly mixed-tasks blocks, the degree of expectancy or uncertainty depends on the number of tasks mixed in the block. When two tasks are mixed, there is a 50% chance for each task to appear in each trial, and when three tasks are mixed, each task has a 33.3% chance of appearing in each trial. Thus, the degree of uncertainty, like the degree of WM load, increases with the number of tasks.

Can this confound compromise our conclusions regarding the WM-load hypothesis? We believe not. If task uncertainty was critical, then it should have affected mixing cost. This claim is based on previous studies that have shown that task uncertainty results in equal slowing of switch and repetition trials (Dreisbach et al., 2002; Ruthruff et al., 2001; Sohn & Anderson, 2001; Sohn & Carlson, 2000). In addition, Monsell, Sumner, and Waters (2003) demonstrated a specific effect of task expectancy on repetition trials. They showed a complete recovery from switch trials after one repetition trial in a predictable sequence (using the alternating runs paradigm), compared with a gradual recovery in repetition trials when the same sequence was unpredictable. Thus, it appears that task uncertainty, like WM load, should increase mixing cost (because repeat trials are not fully recovered and produce slower RTs). Consequently, we argue that the potential confound with task uncertainty would have created an effect which we did not find. Therefore, the fact that we found no effect for WM load cannot be explained by the task uncertainty confound.

The Task Ambiguity Hypothesis

In contrast to the null effect of WM load on switching cost, task ambiguity had an effect, which further supports our conclusions from Experiment 1. Mixing two ambiguous tasks significantly increased mixing cost relative to the mixing of two unambiguous tasks. This effect was mediated by preparation (CTI), meaning that as in Experiment 1, the effect was larger when no opportunity for preparation was provided. In addition, as in Experiment 1, mixing two ambiguous tasks did not increase switching cost relative to mixing of two unambiguous tasks. There was one discrepancy between the results of the current experiment and Experiment 1: In the current experiment, significant mixing cost was found when mixing two unambiguous tasks under the short CTI condition. We suggest that the discrepancy stems from the difference of the between-subjects design used in Experiment 1 and the within-subjects design used in the current experiment. In Experiment 1, the participants in the univalent groups were exposed only to univalent stimuli. In contrast, in the current experiment, when participants performed the blocks of the mixed nonoverlapping tasks, it was after they were previously exposed to the same stimuli in the context of a competing task (they performed single-task blocks of all four tasks before performing any of the mixed blocks). Thus, even in the context of mixed nonoverlapping tasks, each stimulus activated a competing task set, and although this task set was not relevant in the current context, it could still interfere with performance, especially if participants did not get a chance to prepare the task set in advance (see Allport et al., 1994; Waszak et al., 2003). This interference occurred both in repetition and in switch trials, producing mixing cost.

General Discussion

In the current work, we conducted an initial systematic examination of the potential origins of the task mixing cost, an important component of the general alternation cost. Two hypotheses regarding the cognitive origins of mixing cost were investigated. One of them, the task-ambiguity hypothesis, is based on a factor that belongs to a group of *transient* control processes (Braver et al., 2003) that presumably occur during the performance of trials in the mixed-blocks condition and not in the single-task condition. The second hypothesis, WM-load hypothesis, is based on a factor that belongs to a group of *sustained* control processes that presumably discriminate performance in mixed-tasks blocks from performance in single-task blocks. We found clear support for the validity of the first hypothesis but no evidence for the validity of the second.

Our results regarding the WM-load hypothesis are in some way counterintuitive. However, we would like to point out that our evidence against this hypothesis does not implicate that there is not in fact an additional WM load in the mixed-tasks condition. They simply show that this factor does not cause mixing cost. We find it interesting that in the neuroimaging literature, there are findings that could be interpreted as supporting the WM hypothesis. Specifically, some studies found activation in the dorso-lateral prefrontal cortex (DLPFC) during performance of mixed blocks (e.g., D'Esposito et al., 1995; Koechlin, Basso, Pietrini, Panzer, & Grafman, 1999). The DLPFC is widely believed to be involved in WM processes. However, it is possible that the DLPFC activation in these studies was related to the component of WM that is responsible for manipulation of information and not to mere storage (for support, see Smith & Jonides, 1999). Another possibility is that our results can be taken as a further example for the fact that brain activation and RT are not always completely parallels, a fact that is sometimes overlooked in the cognitive science literature. In our case, the discrepancy between RT and imaging results might be an indication that the cognitive system can compensate for the extra WM load, without a cost in response time (for direct evidence for this discrepancy, see Rubin, Brass, Koch, Ruge, & Meiran, 2004).

The evidence in favor of the task-ambiguity hypothesis emphasizes the important role of competition management in task switching and in executive control. Specifically, it shows that, in the task-switching paradigm, the essence of imposing control is not turning task sets on and off but is more accurately captured by metaphors emphasizing the ongoing management of competition between simultaneously activated task sets. We find it interesting that this notion of competition management goes perfectly in line with one of the first theories of executive functions delivered by Norman and Shallice (1986; but see also Desimone & Duncan, 1995; Miller & Cohen, 2001), who suggested that top–down control biases the bottom–up competition between task sets or schemata according to intention.

How Do Our Results Relate to the Existing Theories in the Task-Switching Literature?

Most theories suggest that two types of processes might be indicated by the occurrence of task-switching cost: voluntary, or top–down control processes, and automatic, bottom–up processes. It is widely believed that the voluntary control processes can take place before target presentation, meaning, during the preparation interval. There are, however, different hypotheses regarding the nature of the preparation process and regarding the conditions that require its involvement. De Jong (2000), Mayr and Kliegl (2000, 2003, Meiran (1996, 2000), and Rogers and Monsell (1995) suggested that control is exerted by a process of reconfiguration. By definition, reconfiguration involves a retuning of the system to task set that is different from the one that had just been executed (by, e.g., strengthening activation or the retrieval of specific S-R rules or by shifting the attentional weights to the relevant stimulus dimension). Namely, the reconfiguration is assumed to be specific to trials of task switch and is not needed in trials of task repetition. In contrast, other theories emphasize the role of preparation in both trial types—switch and repetition. These theories focus on processes like task decision (e.g., Dreisbach et al., 2002; Rubinstein et al., 2001; Sohn & Anderson, 2001). By simultaneously exploring three trial types—switch, repetition, and single task—we demonstrated that a large part of the preparation control process that occurs in mixed-tasks block is actually common to switch and repetition trials, and we believe it comprises some kind of task decision process.

In addition, we believe our results shed light on the potential relation between the top–down and the bottom–up processes that are involved in task switching. Specifically, we show these processes not to be independent, which stands in contrast to the findings of Ruthruff et al. (2001) and Mayr and Kliegl (2003). We base this conclusion on the finding that the effect of bottom–up interference on performance (caused by stimulus ambiguity), both in switch and repetition trials, was mediated by the top–down process like task decision. To elaborate, although task decision is a top–down process, it is not immune to bottom–up influence. In particular, when the target is presented before the task set is fully updated (i.e., in the short CTI condition), the information carried by the stimulus influences the task-decision process. After a long CTI, when the task-decision process is likely to be completed, this immunes the system against competing information whose source is the target stimulus, at least to some degree.

Finally, regarding the relation between switching cost and mixing cost, our results show that Braver et al.'s (2003) differentiation between transient control processes, which presumably cause switching cost, and sustained control processes, which presumably cause mixing cost, may not be accurate or at least cannot generalize to every paradigm. We actually propose that a process of task decision, which is transient in nature, is related to mixing cost, whereas the global processes of WM-load management is not. Of course, there are other types of transient and sustained control processes that might be differently related to mixing cost and should be explored in the future. We can mention two factors that might be of interest in future exploration: (a) Task uncertainty, which as mentioned in previous sections of this article, is different in single-task blocks and mixed-tasks blocks; and (b) differential general control effort in single-task and mixed-tasks blocks (see, e.g., Hübner, Futterer, & Steinhauser, 2001; Los, 1996).

Conclusions

We argue that mixing cost is at least as important as switching cost for the understanding of executive control. Although our results are preliminary, they indicate that the transient competition between task sets during set selection is a critical factor in controlling task sets and thus contribute to mixing cost, whereas the factor of sustained WM demand was not supported by our data.

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